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A logical loophole in the derivation of the Bell inequalities

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Abstract. The Bell inequalities are based on a tacit assumption of a common probability distribution that precludes their application to the experiments of Aspect et al. The basic ideas of this argument have already been given in references [1,2], but the present presentation recollects them in a more clear and concise way. The paper supersedes reference [3] which contains an error.

PACS. 03.65.-w Quantum Mechanics

1 The Bell inequalities and their application to the experiments of Aspect et al.

The subject matter of the Bell inequalities and the experiments of Aspect *et al.* hardly needs any introduction [4]. However, the argument has often been blurred by drawing in unnecessary issues, leading to some confusion. We give here an elementary derivation that removes all unnecessary considerations. This will show how elementary the argument is and how very hard it is to question the validity of the inequalities. We consider 4 variables $a_1 \in S$, $a_2 \in S$, $b_1 \in S$, $b_2 \in S$, where $S = \{0, 1\}$. The idea is that 0 correponds to absorption in a polarizer, and 1 to transmission. a_j will correspond to polarizer settings in one arm of the set-up, b_k to polarizer settings in the other arm. There are thus 16 possible combinations for the values of (a_1, a_2, b_1, b_2). By making a table of these 16 combinations it is easy to verify that we always have:

$$\forall (a_1, a_2, b_1, b_2) \in S^4 : \quad Q = a_1 b_1 - a_1 b_2 - a_2 b_1 - a_2 b_2 + a_2 + b_2 \in S. \tag{1}$$

We consider now functions $a_j \in F(V, S)$ and $b_k \in F(V, S)$. The notation means that the domain of the functions is V, while the functions take their values in S. Here V is a set of relevant variables for the experiment. We can call the set V the set of hidden variables, even if some of them may not really be hidden. One can imagine that V could be a subset of a vector space $\mathbb{R}^n$ or of a manifold, e.g. a non-abelian Lie group like SO(3). We have then:

$$\forall \lambda \in V : \quad 0 \leq Q(\lambda) = a_1(\lambda) b_1(\lambda) - a_1(\lambda) b_2(\lambda) - a_2(\lambda) b_1(\lambda) - a_2(\lambda) b_2(\lambda) + a_2(\lambda) + b_2(\lambda) \leq 1. \tag{2}$$

We can now consider a probability density p over V, i.e. $p(\lambda)\, d\lambda$. The function p belongs then to the set of functions $F(V, [0, \infty[)$ with domain V and values in $[0, \infty[$. We further require that $\int_V p(\lambda) d\lambda = 1$. We can now integrate Eq. 2 with p over V. Introducing the notations:

$$p(\alpha_j \wedge \beta_k) = \int_V a_j(\lambda)\, b_k(\lambda)\, p(\lambda)\, d\lambda, \quad p(\alpha_j) = \int_V a_j(\lambda)\, p(\lambda)\, d\lambda, \quad p(\beta_k) = \int_V b_k(\lambda)\, p(\lambda)\, d\lambda, \tag{3}$$

we obtain then:

$$0 \leq p(\alpha_1 \wedge \beta_1) - p(\alpha_1 \wedge \beta_2) - p(\alpha_2 \wedge \beta_1) - p(\alpha_2 \wedge \beta_2) + p(\alpha_2) + p(\beta_2) \leq 1. \tag{4}$$

This is the CHSH Bell inequality used in the experiments of Aspect et al. It is a purely mathematical identity and does not depend on any physical considerations. The probalities are identified with the mathematical expressions for the outcomes of the photon polarization experiments reported by Aspect *et al.*:

$$p(\alpha_j \wedge \beta_k) = \frac{1}{2} \cos^2(\alpha_j - \beta_k), \quad p(\alpha_j) = \frac{1}{2}, \quad p(\beta_k) = \frac{1}{2}, \tag{5}$$

where α_j and β_k are the angles of the polarizer settings in the two arms of the experiment. According to quantum theory the mathematical expressions are the limits of the measured probabilities when the number of registered events tends to infinity, i.e. when the statistics become perfect. For a function $f \in F(\mathbb{N}, \mathbb{R})$, the limit when $n \to \infty$ is defined by:

$$\lim_{n\to\infty} f(n) = F \Leftrightarrow (\forall \varepsilon > 0)(\exists N \in \mathbb{N})(n > N \Rightarrow |f(n) - F| < \varepsilon). \tag{6}$$

Here $f(n)$ would be the measured probabilities after n detection events, F the theoretical expression $\frac{1}{2}\cos^2(\alpha_j - \beta_k)$, and ε the statistical accuracy of the experiment required. An experimentalist has to worry about the statistical precision (and also about instrumental precision). For practical reasons the experimentalist can only reach a reasonable accuracy ε. But this should be well enough to establish beyond any reasonable doubt if the Bell inequality is satisfied or otherwise. We will adopt a mathematician's viewpoint and assume that the expressions $\frac{1}{2}\cos^2(\alpha_j - \beta_k)$ are exact, trusting that at least in principle the experimentalist could prove this to any accuracy ε, by improving the statistics. We introduce thus the assumption (or the act of faith) that the algebra of quantum mechanics is exact. For certain values of $(\alpha_1, \alpha_2, \beta_1, \beta_2) \in [0, 2\pi]^4$, the expressions in Eq. 5 do not satisfy the inequality in Eq. 4. This violation of the Bell inequality shows that the mathematical expressions in Eq. 5 are not compatible with Eq. 4. This seems to confirm Bohr's thesis that the polarizations cannot exist prior to a measurement and must be created by the measurement. But it is then extremely puzzling that we can obtain a definite correlation $\frac{1}{2}\cos^2(\alpha_j - \beta_k)$ because the polarizers can be separated by arbirarily large distances. It looks like the spooky action at a distance Einstein talked about and which has been called entanglement in the aftermath of the experiments. In the experiments of Aspect this issue is tested by ensuring Einstein separation of the detection events in both arms. The solution of this conundrum is in our opinion summarized in the last sentence of Section 3.

2 The tacit assumption

The derivation of the inequality looks unassailable. It is indeed ought to be too simple to possibly hide a logical loophole. But it does! What is not acknowledged is that it contains a tacit assumption, namely that all four quantities $\frac{1}{2}\cos^2(\alpha_j - \beta_k)$ can be obtained from an integration over some set V with a *same common distribution function* p. However, it can *a priori* not be excluded that in reality we can only obtain the quantities $\frac{1}{2}\cos^2(\alpha_j - \beta_k)$ from different distributions p_{jk} according to:

$$\frac{1}{2}\cos^2(\alpha_j - \beta_k) = \int_{V_{jk}} a_j(\lambda)\, b_k(\lambda)\, p_{jk}(\lambda)\, d\lambda \tag{7}$$

rather than:

$$\exists V, \exists!\, p \in F(V, S) \parallel \forall (j,k) \in \{1,2\}^2 : \ \frac{1}{2}\cos^2(\alpha_j - \beta_k) = \int_V a_j(\lambda)\, b_k(\lambda)\, p(\lambda)\, d\lambda. \tag{8}$$

In other words, it is tacitly assumed that the quantities $\frac{1}{2}\cos^2(\alpha_j - \beta_k)$ can all be obtained from *one single common distribution function* p rather than from different distributions p_{jk}. In view of the importance of the subject matter, one may ask for an existence proof of such a unique function p. For a mathematician the need of such a proof would be imperative. Unfortunately, a physicist may remain unfazed by a request for an existence proof. He might have taken the assumption for self-evident and consider the request as futile and faultfinding nitpicking. It would be an ordeal, because such a proof looks completely beyond reach. Perhaps one can get from this a sense of what a frustrating task it was to try to spot the error in the derivation of the Bell inequalities, for people who were upset by their implications. It also looked an unsuperable task, especially because we have no good understanding of how quantum mechanics works.

3 Discussion

It is not my task to prove that assumption Eq. 8 is wrong. That would be a reversal of the charge of proof. All charge of proof is with the authors who proposed the Bell inequalities. I could stop here and wish them ironically good luck. However, I think it will be a more reasonable and respectful attitude to try to provide the physicists in the audience with some arguments why my objection may not be as farfetched as they may think. These arguments may look like a blend of physics and mathematics, but we will show that on close inspection they reveal to be all purely mathematical.

• Our first argument is that the contents of our objection are vindicated by quantum mechanics itself. If one wanted to claim they are wrong, one would thus have to claim that quantum mechanics is wrong. This argument will lead us

straight into the heart of the Bohr-Einstein debate. The starting point of this debate was that when two operators do not commute, they will not have common eigenvectors. This is a purely mathematical truth. An example of this are $\hat{L}_x$ and $\hat{L}_y$. According to Bohr the quantities L_x and L_y do then *not exist simultaneously.* Einstein wanted to prove that this cannot be true and proposed the EPR experiment for two correlated particles, whereby one would measure L_x for one of the particles at $\mathbf{r}_1$ in one arm of the set-up, and L_y for the other particle at $\mathbf{r}_2 = -\mathbf{r}_1$ in the other arm of the set-up. This would then demonstrate that Bohr was wrong. We know now that such a simultaneous measurement is indeed possible, because $\hat{L}_{x_1} = y_1 \frac{\partial}{\partial z_1} - z_1 \frac{\partial}{\partial y_1}$ and $\hat{L}_{y_2} = z_2 \frac{\partial}{\partial x_2} - x_2 \frac{\partial}{\partial z_2}$ do commute when $\mathbf{r}_1 \neq \mathbf{r}_2$. It is only when $\mathbf{r}_1 = \mathbf{r}_2$ that the operators do not commute, with all the consequences non-commutativity may entail. In a sense, Einstein addressed thus the wrong issue, because he no longer addressed operators that were not commuting, but he had been sidetracked by an overinterpretation of the mathematics introduced by Bohr.

We may in fact note that we discover here that the mathematical consequence of the fact that $\hat{L}_x$ and $\hat{L}_y$ do not commute is not that L_x and L_y would not exist simultaneously as Bohr claimed, but that they do not have a common probability distribution function. In fact, we use the eigenfunctions ψ_j of $\hat{L}_j \psi_j = L_j \psi_j$ to define the probability distributions $p_j = |\psi_j|^2$ for a set-up used to measure L_j, based on the Born rule. We will explain below in Section 4 that the Born rule is not something specifically quantum mechanical but a very general result of group theory and therefore completely classical. It is not because L_x and L_y cannot be measured simultaneously in a same location that they would not exist simultaneously in the same location. The difference with what Bohr said may look perhaps subtle but it is very important, because it implies that Bohr has overinterpreted the mathematics. The whole issue is actually a matter that is not at all subject to interpretation, because it is just all pure mathematics. It is the mathematics of the rotation group. These mathematics provide the full explanation about what is going on with the operators $\hat{L}_x$ and $\hat{L}_y$ and their eigenfunctions. Bohr presumably did not know this group theory. Using his physical intuition he cooked up a parallel theory by guessing an *ad hoc* explanation. But physical intuition and mathematical intuition are different things. His parallel theory was at variance with the correct mathematics. It was therefore plain wrong, but nobody knew at that time that group theory was at stake and nobody noticed. Nobody should be blamed for this. Bohr then went on pushing his ideas forcefully. Our rebuttal of the Bell inequality pinpoints his overinterpretation very accuractely. Bohr had a clash with Heisenberg over a very similar overinterpretation in a discussion about the uncertainty relations, which are also tied up with non-commuting operators.

Einstein reasoned on the overinterpretation provided to him by Bohr and was thus right with his intuition that this overinterpretation had to be wrong. We must further point out that the correct interpretation of the consequences of the fact that $\hat{L}_x$ and $\hat{L}_y$ do not commute is completely classical. The operators $\hat{L}_x$ and $\hat{L}_y$ exist in the group theory of the rotation group, which is mere Euclidean geometry. Up to a number of proportionality constants $\hat{L}_z$ corresponds to $\frac{\partial}{\partial \varphi}$ in spherical coordinates (r, θ, φ) in $\mathbb{R}^3$ or polar coordinates (r, φ) in the Oxy plane. The operators $\hat{L}_j$ serve thus to calculate Lie derivatives in three mutually orthogonal directions. They are used to constitute a basis for the tangent space to the group at the identity element as they intervene in the calculation of the infinitesimal generators and of the elements of the Lie algebra. To obtain the Lie derivatives one must use one-parameter sets. These are just completely different for L_x and L_y. The fact that $\hat{L}_x$ and $\hat{L}_y$ do not have common eigenfunctions only means that a rotation around the x-axis cannot simultaneously be a rotation around the y-axis (see [1]). In fact, L_x is not the expression for the x-component of the angular momentum, but the expression for the angular momentum when it is aligned with the x-axis. This is illustrated by the fact that if we call R the rotation around the z-axis that rotates $\mathbf{e}_x$ to $\mathbf{e}_y$, then $\hat{L}_y = R \hat{L}_x R^{-1}$. Hence $\hat{L}_y$ is just the same Lie derivative as $\hat{L}_x$ but in another direction, i.e. for another one-parameter set. Furthermore, one can associate an uncertainty relation with the fact that $\hat{L}_x$ and $\hat{L}_y$ do not commute. But as said, $\hat{L}_x$ and $\hat{L}_y$ are just part of the representation theory of the rotation group and there is absolutely no uncertainty in Euclidean geometry. In summary, the whole algebra belongs just to the theory of the representations of the rotation group in Euclidean geometry and as such to classical mechanics. Hence, even though we are as physicists introduced to the conceptual world of non-commuting operators and Lie groups by quantum mechanics, this world is not "quantum-mechanical" in the sense of non-classical. And to teach us these mathematics, our best guide may perhaps not be a quantum guru.

• Our second argument is another way of showing that the contents of our objection are vindicated by quantum mechanics itself. It is based on one of the ways one calculates probabilities in quantum mechanics. In fact, what one does is set up a Schrödinger equation, solve it to find the wave function ψ and then stipulate $p = |\psi|^2$, according to the Born rule (which is not non-classical as we will discuss in Section 4). It must be mathematically obvious that one can in general not assume that the solution ψ_1 of a first Schrödinger equation will be equal to the solution ψ_2 of a second different Schrödinger equation. One can therefore in general not carry over probabilities that are valid for one set-up to another set-up. But this is exactly what the ansatz of a common probability distribution for the four different correlation experiments does. Of course this argument is completely equivalent to the first one. We just discover it

here in a different guise.

• A third argument which one may consider as more classical (see below) because it does not depend on the Born rule is that the definition of a probability depends on a full context and a protocol as one discovers by the paradox of Bertrand. One can connect this to the previous arguments. When we solve a Schrödinger equation, we take into account the necessity of outlining the full context by coding it into the boundary conditions, perhaps even unwittingly. It is well known that solutions of a Dirichlet problem can heavily depend on the boundary conditions. This may be used to illustrate the profound impact boundary conditions may have. This third argument illustrates perhaps further the fact that the first two arguments are not quantum mechanical, but purely classical. What would not be classical is Bohr's overinterpretation, but we can appreciate now that this is wrong.

• The three arguments given up to now recollect what we already developed in [2]. A fourth argument consists in referring to Gleason's theorem which is obviously purely mathematical (and also does not depend on the Born rule). But this is of course very similar to the previous arguments. We list these four arguments as different arguments only because they might look different at first sight. When there is a hard nut to be cracked in order to solve a mathematical problem, one will forcedly hit it whatever the road one takes in trying to solve the problem. Call it a conservation law for hard nuts. By changing the approach we may only discover the hard nut in a different guise.

In summary, we have been aware of this kind of objections for a long time but they have been overlooked in deriving the inequalities, perhaps because it was considered that the objections were quantum mechanical and not classical such that one should not consider them in something that was supposed to represent classical thinking as opposed to quantum mechanical thinking! But what is here associated with classical thinking is only poor mathematical thinking based on "physical intuition" applied to problems that are purely mathematical! What is wrong and pollutes the whole debate are the overinterpretations of the mathematics the Copenhagen interpretation is teeming with and are supposed to define "quantum intuition".

We must now take on a further element of intuition which is wrong. We must become aware of the fact that the Copenhagen interpretation has brainwashed us with the idea that wrong intuition corresponds to classical mechanics and the correct approach to quantum mechanics. The reader may have the intuition that it should be possible to enlarge the sets V_{jk} to a set V in such a way that p will engulf all probabilities p_{jk}. That intuition is contradicted by what we pointed out above. In trying to follow that intuition and to define a common distribution function p one will run into all kinds of difficulties, which is normal because they are there to prevent us from deriving a contradiction from the mathematics. But as physicists we have been taught to take our strides with mathematical rigor, such that we are prone to make some booby traps go off. Very often we get away with our lack of rigor, but not this time. This time we have paid very dearly.

It is obvious that if we turn a polarizer, we turn a distribution of molecules. If one believes in hidden variables, then this distribution of molecules must be part of the hidden variables. Extending generously the set V to allow for all possible angles would render p a function of an infinite set of variables. Defining p may require the axiom of choice, which is responsible for the Banach-Tarski paradox. It would also render the statistical weight of a single angle zero. What one can do in physics to avoid the zero probabilities is to select a polarizer angle a by introducing a delta measure δ_a. But the derivation given above is based on functions, not on distributions. To circumvent this problem, one can introduce test functions T_u that in the sense of distributions converge to δ_a. An example is T_u, with $u \in]0,1[$:

$$T_u(x) = \begin{cases} \frac{1}{2u} & \forall x \in [-u,u] \\ 0 & \forall x \notin [-u,u] \end{cases} \quad \Rightarrow \quad \lim_{u\to+0} \int_{-\infty}^{+\infty} f(x)\, T_u(x)\, dx = f(0), \quad \lim_{u\to+0} T_u \rightsquigarrow \delta_0 \tag{9}$$

But how do we accomodate these test functions into the derivation given above? One cannot include the test functions for δ_{a_j} and δ_{b_k} into p else we will fall back onto the original p_{jk}. We may note in passing that the selection of an angle is actually not a probability. We are thus forced to consider $T_u(x)$ as outcomes of events. But the test functions cannot be considered as outcomes of events because they do not belong to $F(V,S)$, such that the inequality in Eq. 2 will no longer be granted. In the limit procedure the test functions even take arbitrarily large values as $\lim_{u\to 0+} T_u(0) = \infty$. The repair procedure does thus not fit into the scheme for the derivation outlined above. Its putative proof is not covered by the derivation and remains to be found. As explained above, quantum mechanics and other arguments indicate that the putative proof will just not exist. Other scenarios *must* therefore lead to similar problems, because there is also a conservation law for mathematical no-go theorems. We would finally like to point out that the angle $\alpha_j - \beta_k$ is *non-locally defined*, without any need for signalling and without any violation of relativity, such that this non-locality is not an issue [2]!

4 The Born rule

We may think that the Born rule is eminently non-classical. But the Born rule is an unavoidable consequence of the fact that vectors and four-vectors are "quadratic" rank-2 expressions in terms of spinors in the Lorentz group and in the rotation group [5]. It is just group theory. Let us give the reader some feeling for this idea. For the idea that the unit vectors $\mathbf{e}_x$, $\mathbf{e}_y$ and $\mathbf{e}_z$ of $\mathbb{R}^3$ are quadratic expressions of the spinors of the rotation group SU(2), we refer the reader to reference [5], more specifically to Eqs. 30-32 in this working document. These equations tell us that for $\mathbf{e}_x = (x_1, y_1, z_1)$, $\mathbf{e}_y = (x_2, y_2, z_2)$, $\mathbf{e}_z = (x_3, y_3, z_3)$, we have:

$$\begin{aligned} x_1 &= \tfrac{1}{2}(\xi_0^2 - \xi_1^2 + \xi_0^{*2} - \xi_1^{*2}), & y_1 &= \tfrac{\imath}{2}(\xi_0^2 + \xi_1^2 - \xi_0^{*2} - \xi_1^{*2}), & z_1 &= -(\xi_0\xi_1 + \xi_0^*\xi_1^*), \\ x_2 &= \tfrac{\imath}{2}(-\xi_0^2 + \xi_1^2 + \xi_0^{*2} - \xi_1^{*2}), & y_2 &= \tfrac{1}{2}(\xi_0^2 + \xi_1^2 + \xi_0^{*2} + \xi_1^{*2}), & z_2 &= (\xi_0\xi_1 - \xi_0^*\xi_1^*), \\ x_3 &= \xi_0\xi_1^* + \xi_0^*\xi_1, & y_3 &= \imath(\xi_0\xi_1^* - \xi_0^*\xi_1), & z_3 &= \xi_0\xi_0^* - \xi_1\xi_1^*. \end{aligned} \tag{10}$$

By covariance, any vector $\mathbf{r} \in \mathbb{R}^3$ is a quadratic expression in terms of spinors. To generalize Eq. 10 to vectors $\mathbf{r}$ that are not unit vectors, we must use generalized spinors $\boldsymbol{\psi} = \sqrt{r}\,\boldsymbol{\xi}$. This can be generalized to Minkowski space-time. The four basis vectors of Minkowki space time are quadratic expressions of Lorentz spinors, and four-vectors in general quadratic expressions of spinors. The reader can get a feeling for this generalization from reference [5] where we generalize the results for the rotation group in $\mathbb{R}^3$ to rotation groups in $\mathbb{R}^n$.

We know from relativity that charge and current density ρ and $\mathbf{j}$ are part of a charge-current density four-vector $(c\rho, \mathbf{j})$ for electric charges. A probability density is part of an anologous four-vector for "probability charges". We can call this the probability charge-current density four-vector. To stress the analogy, we will write this four-vector in what follows also as $(c\rho, \mathbf{j})$, such that we will use both ρ and p to refer to the probability density.

In the representation SL(2,$\mathbb{C}$) of the Lorentz group a four-vector $(c\rho, \mathbf{j})$ is expressed as $c\rho\mathbb{1} + \mathbf{j}\cdot\boldsymbol{\sigma}$ (see e.g. reference [1], Chapter 4). It transforms under Lorentz transformations $\mathbf{L} \in$ SL(2,$\mathbb{C}$) according to:

$$c\rho\mathbb{1} + \mathbf{j}\cdot\boldsymbol{\sigma} = \begin{bmatrix} c\rho + j_z & j_x - \imath j_y \\ j_x + \imath j_y & c\rho - j_z \end{bmatrix} \rightarrow \mathbf{L} \begin{bmatrix} c\rho + j_z & j_x - \imath j_y \\ j_x + \imath j_y & c\rho - j_z \end{bmatrix} \mathbf{L}^\dagger \tag{11}$$

Four-vectors are bilinear covariants, i.e. quadratic expressions in terms of spinors, which transform linearly: $\boldsymbol{\xi} \rightarrow \mathbf{L}\boldsymbol{\xi}$ under Lorentz transformations. The quadratic transformation law Eq. 11 is just another manifestation of the fact that four-vectors are quadratic expressions of spinors. One therefore often states that a spinor is the "square root of a vector".

The ultimate non-relativistic limit corresponds to $\mathbf{v} = \mathbf{0}$. We have then $\mathbf{j} = \mathbf{0}$ and $\mathbf{L}$ becomes a rotation $\mathbf{R}_0$, as the Lorentz group reduces to the rotation group SU(2), which is embedded within SL(2,$\mathbb{C}$). The four-vector reduces then to a scalar, because it becomes $(c\rho, \mathbf{0})$. For probability charges this scalar has to be positive-definite and of degree 2 in terms of a spinor $\boldsymbol{\xi}$, because, as said, four-vectors are quadratic expressions of spinors. The most simple quadratic expression for such a positive-definite scalar that comes to one's mind is $p\,[\,\boldsymbol{\xi}^\dagger\boldsymbol{\xi}\,]$. Here:

$$\boldsymbol{\xi} = \begin{bmatrix} \xi_0 \\ \xi_1 \end{bmatrix}, \tag{12}$$

is the spinor corresponding to the SU(2) rotation matrix:

$$\mathbf{R}_0 = \begin{bmatrix} \xi_0 & -\xi_1^* \\ \xi_1 & \xi_0^* \end{bmatrix}. \tag{13}$$

We have then $\boldsymbol{\psi} = \sqrt{p}\,\boldsymbol{\xi}$, and $p = \boldsymbol{\psi}^\dagger\boldsymbol{\psi}$. Well, to be quite honest, the solution is not unique. We could actually also propose $\mathbf{L}^\dagger\,\boldsymbol{\psi}^\dagger\boldsymbol{\psi}\,\mathbf{L}$, for some $\mathbf{L} \in$ SL(2,$\mathbb{C}$), but this would only correspond to a change of basis: $\boldsymbol{\psi} \rightarrow \mathbf{L}\boldsymbol{\psi}$ by a group transformation. The argument given here is only a sketch of what would be a mathematically complete and rigorous derivation. The full derivation within the context of the Lorentz group would be quite involved and represent several pages of tedious algebra. But we think the idea is already clear from this. In fact, it seems obvious that there is no other alternative when we ask for a quadratic expression of $\boldsymbol{\psi}$ that should be positive-definite. What the expression should be for $\mathbf{v} \neq \mathbf{0}$ must forcedly be a generalization of this concept. The wave function $\boldsymbol{\psi}$ is thus a spinor field, that takes the values $\boldsymbol{\psi}(\mathbf{r}, t)$. The derivation above is valid within the context of the Schrödinger equation, where we neglect the relativistic effects of boosts. In a moving frame, we would have $\rho \rightarrow \gamma\rho$, but as $\gamma \approx 1$, its expression will not change in a non-relativistic context. We will only have also a current density $\mathbf{j} \neq \mathbf{0}$ when $\mathbf{v} \neq \mathbf{0}$.

In the Schrödinger equation we actually only treat the phase of $\boldsymbol{\psi}$, considering the other components to be constant, such that we can drop them tacitly from the formalism. In other words, we replace:

$$\boldsymbol{\xi} = e^{\imath\varphi} \begin{bmatrix} 1 \\ 0 \end{bmatrix} \to \xi = e^{\imath\varphi}, \quad \boldsymbol{\xi}^\dagger \boldsymbol{\xi} \to \xi^* \xi. \tag{14}$$

such that $\boldsymbol{\psi} \to \psi = \sqrt{p}\, e^{\imath\varphi}$ with $p = \psi^* \psi$. Within the Dirac theory, the definitions must be adjusted, but the expressions always follow from group theory.

In quantum mechanics one shows that the probability charge-current four-vector obeys a continuity equation. The expression for this continuity equation is fully covariant, because it is expressed in terms of the "scalar product" in Minkowki space-time of the four-gradient and the probability charge-current four-vector. From this continuity equation we can derive the Born rule by *inductive reasoning*. This may look more familiar and persuasive to a physicist. But even if it resonates better, it is less rigorous and less all-encompassing than the *deductive argument* given here and which is purely mathematical. In fact, the feeling among physicists would be that there is no watertight proof of the Born rule, that the rule is highly plausible and that it has been confirmed by experiment. This is because they have tried to derive the rule inductively using physical intuition, ignoring that the rule is a purely mathematical result which can be proved deductively. It is the same story as with Bohr's interpretations we denounced above. We are not exaggerating when we say that the Copenhagen is teeming with ersatz physical pseudo-derivations of purely mathematical facts. Many quantum paradoxes are just dreadful off-shoots of bogus "physical intuition" that has been spilled all over the place. It is an absolute disaster as the debacle of the Bell inequalities painfully illustrates. Group theory contains special little traits, like the use of automorphism groups [5], that are beyond physical guessing and render it impermeable to layman intuition. The Born rule is thus pure group theory and pure classical mechanics.

Epilogue. We noted that we could have stopped our rebuttal of the Bell inequalities at the end of Section 2. All the additions are just forced upon us by physicists, who are not receptive to mathematical rigor, such that we are forced to anticipate that they could pooh-pooh the objection in Section 2 despite the fact that it is a completely pertinent, terrible blow to the derivation of the Bell inequalities. Because the algebra agrees with experimental data, they fail to conceive that their textbook dogma could be wrong. But that the algebra is exact does not exclude that the dogma with all its over-interpretations of that algebra could be wrong, turning their science into a religion. They would scream blue murder that they do not understand quantum mechanics but schizophrenically reject papers out of hand without reading them when they do not comply with that religion, the very religion they are complaining about, the religion that is a conceptual swamp due to its over-interpretations of the algebra.

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5. An introduction to SU(2) and SO(3) is given in `https://hal-cea.archives-ouvertes.fr/cea-01572342`, combined with Chapter 3 of [1]. One cannot address quantum mechanics without a perfect mastery of spinors and group theory.